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Title	Traumatic brain injury in a rural indigenous population in Canada: a community-based approach to surveillance
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Reviewer 1	Paul Hasselback MD MSc FRCPC
Institution	Island Health Medical Health Officer, Nanaimo, BC
General comments (author response in bold)	<p>This is a strong study and deserves serious consideration; the detailed comments are designed to enhance quality and not detract from the excellent work done to date.</p> <p>This study aimed to describe the traumatic brain injury (TBI) of Health Region 18 (HR-18) in Quebec compared to two neighboring regions and the entire province.</p> <p>The authors seem to have worked closely with the First Nations and Indigenous Peoples' authorities and have emphasized equity in their analysis in the best traditions of public health research. Compliance with ethical research guidelines appears to be satisfactory noting that there is no current standard for demonstrating compliance with TCPS II Chapter 9 conditions. The authors are commended for adhering to these protocols.</p> <p>1. It would be beneficial for the editors to be aware of how this data has been shared with the members of the communities beyond the James Bay/Cree health board and how the community has agreed to the sharing of the information to ensure data are not released to the scientific community without local knowledge sharing.</p> <p>Thank you for this comment. The Cree Board of Health and Social Services of the James Bay (CBHSSJB) has been directly working on the development of this project since it began. The information of this study has been disseminated to many organizations, community stakeholders and community members through many different avenues over the last months, since the goal of this study was to increase the awareness of TBI in Eeyou Istchee and for different community organizations to develop prevention strategies.</p> <p>2. The authors have stated that they consider two indigenous populations to be more similar to each other than the general population with regard to culture, environment and socio-economic status and this assumption is used to "partially control for unmeasured covariates", a statement that is not justified without corroborating data. The sentiment is appreciated, the text should be more precise.</p> <p>We have omitted this statement since our study did not have corroborating data.</p> <p>There is a commendable focus on equity throughout the paper.</p> <p>Thank you.</p> <p>3. As a general comment, at times the use of words is imprecise, and while this may be resolved through the editing process, it would be of assistance of have the manuscript reviewed by a writer prior to submission.</p> <p>Throughout the revision process, we have focused on improving the precision of terms.</p> <p>Specific comments:</p> <p>Methods</p> <p>This is a population based retrospective cohort study of hospitalized TBI cases from HR-18, with data & epidemiological descriptions of HR-17 and HR-10 used as comparators.</p> <p>4. The use of hospitalization data for case finding is necessarily biased in that fatalities, non-admitted or referred cases may be missed regardless of severity; the approaches used to measure and mitigate this bias are as reasonable as can be expected, and have been duly noted as a weakness in the study.</p> <p>Thank you.</p> <p>5. Missing data has been imputed assuming that it is missing at random which may not be the case in assault related TBI given the possibility of criminal charges. The methods described on P10, lines 7-14 are only acceptable for truly random missing data. Biases might include a tendency to less completed information where trauma involves assault, alcohol, drugs etc. – which are associated with a key outcome indicator in the study group. While not necessary, a sensitivity analyses that tended towards non-random completion of missing data where such risks were involved might have an impact on the outcome. At least a comment that the missing data assume random omission might impact the study outcome. See the note below on alternative methods for addressing missing data.</p> <p>See below (Point 8) and the note to the editor's comment above.</p> <p>The sensitivity analysis to control for rural versus urban hospitalization rates is a nice angle and helps to address a clear potential bias.</p> <p>Results</p> <p>6. 55% of patients are stated to have been intoxicated with alcohol but no data is presented, specifically at what level and what measurement was used to define "intoxication". This might be any alcohol, 0.05 or 0.08 and if possible should be specified what level and how determined.</p> <p>The methods section describes that any physician chart note that indicated that the patient was intoxicated with alcohol was used to define "intoxication".</p> <p>7. Table 1: ICD 959 (Head Injury) does not appear to have been included in the list of diagnostic codes searched, nor facial bones fracture (ICD 802), both of which can be associated with TBI. Clarification of this possible coding discrepancy would be helpful.</p>

	<p>This is a very good point. We did not include this ICD code since we wanted our data to be comparable to the INSPQ report, where this code was not used. In the limitations section, we have added that omitting this code may lead to an underestimation of our rates.</p> <p>8. P35 Figure 2. 20 cases had no charts available and should not be treated as random for imputation purposes when it is stated that assaults are more likely to cause TBI. This again speaks to the missing data mechanisms used in the study for which a bias may exist. (an alternative method to demonstrating this is to demonstrate that for those cases with missing data, there are no significant differences on characteristics which were likely used for the imputation prior to doing the imputing—this analysis has not been provided)</p> <p>The patients with missing data and the patients with full data had similar distributions of covariates that we had measured completely. We have reanalyzed our data with a fuller imputation model, which includes age, mechanism of injury, year of injury and geographical zone of injury to limit the amount of bias related to missing data. With this approach, the conclusions of our regression models did not change compared to our previous analysis. We have also added to the limitations section that the missing data mechanism may not be fully accounted for and residual bias may be present.</p> <p>We appreciate the hypothesis that patients involved in assaults may be more likely to have missing data. However, the missing data for the chart review were for entire charts that were not accessible and not only specific details within the chart. As such, even if patients were involved in assaults they should still have accessible charts for their hospitalization. The proportion of patients with missing data that were involved in assaults was very similar to patients with complete data (30% v. 34%). Therefore, the missing data mechanism is not completely clear but as above, we tried to adjust for it based on the variables that were measured completely.</p> <p>9. P44 Supplementary Figure s7: No patients hospitalized from snowmobile collisions wore a helmet? This needs double-checking as I find it hard to believe that not one serious snowmobile accident involved a subject not wearing a helmet, particularly given the need for temperature protection in winter.</p> <p>After rechecking our primary data and reviewing the TBI database we used to validate our chart review, we confirm that no patients involved in a snowmobile accident were wearing a helmet. A previous study in Quebec also supports this finding. (Su W, Hui T, Shaw K. All-terrain vehicle injury patterns: are current regulations effective? J Pediatr Surg 2006;41:931-4.)</p> <p>10. Table s9: The presentation of multiple regression findings needs to clearly indicate what the results mean. Perhaps a new column which is a short written statement of the interpretation so that there is clarity for the reader and for those wishing to reference.</p> <p>Table 4 of the main manuscript has a column that interprets the findings of the association measures we are reporting.</p> <p>11. Table s9f P51: despite the discussion previously, there is no comment on non-significance of alcohol intoxication in this regression.</p> <p>We have discussed this finding in the discussion section: “Third, in the present study, alcohol intoxication at the time of injury did not seem to be an important factor in terms of functional outcome (Table S7d). Still, the precision of this estimate was quite poor and firm conclusions cannot be drawn. Previous studies have shown that up to 50% of traumatic brain injuries occur in the context of alcohol intoxication, which is not significantly different from the rate we reported of 44.3%.[41] Still, alcohol use has been shown to be an important risk factor for traumatic brain injury recurrences, which ultimately lead to poorer functional outcomes [42]. As such, addressing alcohol abuse remains important in our study population as it does in other populations, and further investigations on this topic are warranted.”</p> <p>Discussion.</p> <ul style="list-style-type: none"> - The broad conclusions of the paper are justified given the data and interpretation of the results. - The authors are to be commended for challenging current provincial prevention strategies and highlighting the shortcomings of that research. - Also commendable is the discussion of the receipt of rehabilitation services depending on cause of TBI, an important point of equity. <p>Thank you.</p> <p>12. Statement that different geographical regions are important determinants of TBI is overstating the association described by the results.</p> <p>This statement has been omitted. We have focused on stating that the remote geographical zone was associated with the highest injury rates and severities:</p> <p>“Living in the remote geographical environment was associated with the highest hospitalization rates and the most severe injuries.”</p>
Reviewer 2	Dr. Aaron Johnston
Institution	New Westminster, BC
General comments (author response in bold)	<p>This is a very interesting paper. The potential limitations of the methodology; relying on ICD9 coding, possible missed fatal cases and retrospective GOS calculation were well described.</p> <p>The data shows some interesting and important differences in TBI based on geography.</p> <p>Thank you for the feedback.</p>
Reviewer 3	Michael E. Green, MD, MPH, CCFP, FCFP
Institution	Associate Professor, Depts of Family Medicine and Public Health Sciences, Queen's University; Director, Centre for Health Services and Policy Research; Associate Director, Centre for Studies in Primary Care; Scientist, Institute for Clinical Evaluative Sciences

General comments (author response in bold)	<p>We appreciated the comments and feedback.</p> <p>This paper on the epidemiology of TBI in both indigenous and non-indigenous regions of northern Quebec highlights the importance of regional or community level data to help drive policy recommendations, in this case for TBI. They demonstrate very nicely and with sound statistical methods the significant differences between all of their study regions (which are all rural and northern) and the rest of the province as well as difference between the three selected regions. By combining primary data collection through chart review with administrative data they were able to move beyond describing incidence and prevalence and discuss outcomes and the predictors of this as well. I think this is an important topic and the methods are generally sound.</p> <p>The paper does however need some significant revisions.</p> <p>1 - There is far too much important core information in the “supplemental” files attached. A reader should be able to follow the paper without having access any of the supplemental files and this is not currently easy to do. Some key information that really should be in the main document is in these files, while some information in the suggested main tables could be moved back. For example. The list of ICD codes in Table 1 could be moved to supplement as only a small number of very keen methodologists would really be interest in this. In contrast, supplemental figure s2, the flow chart for inclusion in the chart abstraction is essential information that needs to be in the main paper. The special status of the Cree Board of Health and Social Services of James Bay within Quebec should be described in the background section of the paper to orient readers and used to explain why HR-18 is made up of communities nested within HR-10 (Most) and HR17(one only). I think the data in Table s4 should also be included in the main paper, rather than as a supplement.</p> <p>All of these suggestions have been considered and incorporated in our manuscript. Regarding the special status of the CBHSSJB, we have included a description in the Methods to keep the Background section short.</p> <p>2 - In table 2 only the crude incidence is presented along with the adjusted IRR, with the standardized rates by year presented in a figure. Given the relatively small population of the regional data it exhibits wide annual variation, which is to be expected. I don't know that this figure adds much over reported a single age standardized incidence rate for the study period. If it is intended to explore the possibility of time trends then perhaps consideration should be given to using a 3 or 5 year rolling average for this figure to smooth out the large swings caused by having a small population. Alternatively the current figure could be a supplemental file and a line for reporting the age standardize incidence added to table 2. Additionally they may want to comment on which rate – crude or standardized – is most important for planning local policy, particularly as HR 18, like many highly indigenous regions may have a significantly different age structure from the general population.</p> <p>We have changed this Figure (now Figure 1) and have included a 4-year rolling average.</p> <p>3 - For the analysis based on the chart review I would suggest including more of the summary data currently reported in the text and in supplemental table s6 as a table in the main paper.</p> <p>This table is now included as Table 3 in the main manuscript.</p> <p>4 - I have one additional comment on the main conclusion. In the abstract I think they could safely note that for all of these regions there were significant differences in root causes from the general population, highlighting the importance of regional or community level data to inform prevention programs for both rural and indigenous populations. In fact, their data shows that drilling down further can even highlight community specific priorities within a region. The language used to summarize this in the abstract is confusing and should be revised.</p> <p>We have made our main conclusion in the abstract more precise.</p>
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